

I may, however, be permitted to make a few remarks arising out of Mr. Grenfell's paper.

According to my view, a *nucleus* is a body that has a stronger attraction for the gas, or the vapour, or the salt, of a supersaturated solution than for the liquid that holds it in solution.

Nuclei, with certain limitations, cease to be such when made *chemically clean*.

A body is *chemically clean*, the surface of which is entirely free from any substance foreign to its composition.

Thus oils and fatty bodies are *chemically clean*, if *chemically pure*, and containing no substance, mixed or dissolved, that is foreign to their composition.

The limitations above referred to are two: (1) the oils, &c., when *chemically clean*, do not act as *nuclei* while in the mass, such as a lens or globule; but these oils, &c., whether *clean* or not, in the form of thin films, act powerfully as *nuclei*; (2) a liquid, at or near the boiling point, is a supersaturated solution of its own vapour, and a porous body, such as charcoal, pumice, &c., whether *clean* or not, is a powerful *nucleus* in separating vapour.

I have on several occasions taken the liberty of opposing M. Gernez's views as to the action of *nuclei*. He supposes (1) that supersaturated gaseous solutions (soda water, seltzer, &c.) give off their gas to *nuclei* by virtue of the air that these latter introduce into the solution: in other words, gas must escape into air, and the function of the *nucleus* is to carry down air; hence rough bodies act better as *nuclei* than smooth ones. I have shown (*Phil. Mag.*, August 1867) in a series of twelve experiments, that air is not a *nucleus*, and that rough bodies are inactive, if *catharised* or made *chemically clean*. A rat's-tail file, for example, is a good *nucleus*, because it holds between its teeth not air, but that filmy kind of matter that is powerfully *nuclear*, and it is not easy to clean a body of this kind; but when *clean*, it is quite inactive. So, a flint stone that has been exposed to the air, or handled, acts as a powerful *nucleus*, but when broken, the newly-fractured surfaces are inactive, because *chemically clean*. And such surfaces are inactive, because the gaseous solution adheres to them as a whole; whereas, if a *clean* body be handled or exposed to the air, it becomes covered, more or less, with filmy matter, to which the gas adheres more strongly than the liquid does, and hence there is a separation.

There is, I think, abundant proof that air is not a *nucleus*, its function, if it have any, in this class of phenomena, being that of a *carrier of nuclei*. Proof also is wanting, I imagine, that when a *nucleus* determines the crystallisation of a supersaturated saline solution, a salt of its own kind is present. When M. Gernez so laboriously prepared his *nuclei*, so as to free them from salt, he did not perhaps reflect that he was making them *chemically clean*. Of course I fully admit that, in general, a salt of the same kind as the solution, acts as a powerful *nucleus*; but in order for it so to act it must adhere more strongly to the saline than to the liquid portion of the solution. It may even happen that a crystal of the same kind, and of the fully hydrated salt, has no *nuclear* action, because it is in a perfectly *catharised* condition. And here I must refer to the objection raised by Dr. De Coppet, that in one of my forms of showing this experiment, the hydrated crystals, say of magnesian sulphate, being introduced into the neck of the flask while the solution was boiling, and so left in the covered flask while the solution cooled, such crystals become so changed by the heat as no longer to represent the normal salt, so that when lowered into the solution they formed a different salt, and hence were no test of the point in question, as to whether a salt of the same kind may be rendered inactive as a *nucleus*. I admit the criticism to be just, but in my original account of the experiment (*Phil. Trans.*, 1868, p. 665) I did not rely upon one form only. Highly supersaturated solutions in clean tubes, plugged with cotton wool, were put, when cold, under the receiver of the air-pump, and left for some time *in vacuo*, over sulphuric acid, the effect of which was to produce crystalline crusts of the normal salt on the surface, and these by shaking fell through the solutions without acting as *nuclei*; whereas on removing the cotton wool in the presence of air, the solutions crystallised immediately into a solid mass. So also by keeping supersaturated solutions during some months, water escapes through the cotton wool, and a crystalline crust of the normal salt creeps up the air-filled portion of the tube, and this has no *nuclear* character, because the adhesion between it and the solution is perfect.

So necessary is the action of a *nucleus* in determining crystallisation in these solutions, that, if care be taken to exclude

nuclei, highly supersaturated saline solutions may, by reduction of temperature to 0° F., or from that to -10° F., be made solid, and by placing the tubes in snow and water at 32° F., the solids rapidly melt into clear bright solutions, without any separation of salt. These effects may be shown any number of times; but whether the solution be solid or liquid, if the cotton wool be removed, crystallisation always sets in, in the case of the solid during the melting, while in that of the liquid the effect is immediate.

With respect to the editorial note that the solutions of hydrated salts contain the anhydrous salt, I have shown in the paper last quoted, and with still greater elaboration in the *Chemical News* for Dec. 10th, 1869, that such is the case with respect to sodic sulphate. I insist on this point, as it is one of first-rate importance in considering the theory of supersaturated saline solutions. I endeavour to prove that it is the anhydrous salt in solution, by showing that at various points of the scale a sudden lowering of temperature produces a shower of the well-known octahedral crystals of the anhydrous salt. I also explain in my original memoir, that it is necessary for these crystals to be deposited before the modified 7-atom salt can be formed, and that even when there is a copious deposit of this salt the liquor above it is not, as Löwel supposed, the mother liquor of the 7-atom salt, but it is still a solution of the anhydrous salt. And more than this, when the sudden change in the curve of solubility takes place at 33° or 34° C., and there is, according to Gay Lussac's supposition, a change in molecular condition, it is still the anhydrous salt that is in solution.

There are several other points that might be enlarged on, but that I fear to trespass further on your valuable space.

Highgate, N.

CHARLES TOMLINSON

Astrology

THE belief in astrology which still prevails among the English lower classes to a much larger extent than is supposed, will derive a fresh impulse from the happy guesses which have been made by the editor of "*Moore's Almanac*" in his issue for the current year. The hieroglyphic with which it is illustrated is less vague than usual, and represents two eagles fighting in the air, and on the plains beneath them hosts of armed men (in decidedly foreign uniforms) engaged in a bloody struggle. Lest the point should be missed, the prophet begins the forecast of the year with the distinct assertion that there will be war between France and Prussia, and that the month of July will be especially disastrous to the Emperor Napoleon. Thus far events have coincided with the voice of the oracle, and seem to confirm the poet's view that

"The warrior's fate is blazoned in the skies."

But we have yet to see whether "in October the King of Prussia (if living) will meet with defeat, and the ex-King of Hanover recover some of his prestige, if not his throne also." M. Comte would have us deal tenderly with astrology, because it was, in his opinion, the first systematic effort to frame a philosophy of history out of the apparently capricious phenomena of human actions. In theory we may do so, but astronomical science is hardly likely, for the sake of sentiment, to treasure up the discarded swaddling clothes which for so many centuries impeded its onward progress.

Norton Canon, Weobley

C. J. ROBINSON

On Volcanoes

HAVING only last night returned from Norway, I was not aware before to-day that No. 40 of NATURE (August 4) contained "an outline" of a lecture on volcanoes delivered by me in St. George's Hall, Langham Place, on the 19th June (not 9th as therein stated) last.

Although I cannot but feel highly flattered at the length of this notice, I must regret that the author of this "outline," who, strangely enough, signs himself by my name, has, as will be seen upon reference to the text of my lecture as reported in the *Geological Magazine* for July, omitted every word which could convey to the reader the remotest idea of the object of the lecture itself, or the conclusions arrived at from the evidence brought forward. Just as a man without life is but a corpse, neither can a mere string of facts be called even the "outline of a lecture," when we have only the body without the spirit.

The object of my lecture was to institute a comparison between the relative magnitudes of the operations of internal and external forces in determining the main external features of

our globe, and the conclusion arrived at was, to "grant the first rank to the internal, volcanic, or cataclysmic agencies, since, had it not been for their operations, our globe would still have remained a comparatively smooth sphere, surrounded by its external envelope of water, with no visible land for the rivers to traverse or the rain and ice to disintegrate and wear away," &c.

In order that your numerous scientific public may not be led to judge of the lecture by this outline, I trust to your good will in asking you to insert these remarks in your next number.

DAVID FORBES

11, York Place, Portman Square, Aug. 22

A Vivid Mirage

THE illusion known as the mirage is, I believe, not unfrequently observed in the British Isles; but the vividness with which it was displayed on the present occasion will, I trust, be a sufficient apology for troubling you with this letter.

The land bordering the River Nene is protected by banks of from twelve to fifteen feet in height, enclosing a space called the "Wash," which receives the flood waters. It was from one of these banks that the appearance in question was observed, nothing unusual being seen from the level of the fen. I may mention that the Wash at this season is as dry as any other portion of the land.

The day (August 12) was hot, the sky cloudless, and a strong N.E. wind was blowing. About eleven o'clock the phenomenon was first noticed. To the eastward a dark line of trees, some eight miles distant, stood out in bold relief against the clear sky, and in front of this a shining line of silvery brightness was seen, which gradually widened until about twelve o'clock it presented the appearance of a broad expanse of water, ruffled into waves on its near side, but perfectly calm and clear toward the horizon where the line of trees was beautifully reflected on its surface. As I had been approaching the scene all this time, the expanding of the lake appeared perfectly natural, and I could scarcely help thinking the river must have overflowed during the night and drowned the "Wash." This, of course, I knew to be quite out of the question, but the semblance was so perfect that it required an effort to believe that it was but an illusion. Its shores were clearly defined, little bays dimpled it, tiny headlands jutted out from it, and the waves were seen rising and falling with life-like exactitude. The whole appeared quite stationary, and as I approached the spot it gradually faded away, until nothing but a thin blue haze beneath the trees remained, and this at length dissolved.

On looking behind me (*i.e.* westward) another mirage seemed forming, which increased in apparent extent as I went farther from it. In this case the illusion was, if possible, more perfect than in the last, and the comparatively high land of Whittlesea rose like an island from the shining sea. Vehicles passing along the road seemed floating on its surface, their dark drawn-out reflections showing vividly against the sun-lit water, which, in this instance, was quite calm. How long this illusion lasted I know not, but when, about two o'clock, I quitted the bank it was still very distinct.

The dead level of the fen, and the bright sunlight falling upon the parched land, from which the heated air rose tremulous as from a hot plate, render this district peculiarly favourable to the production of such effects. For the accuracy with which the appearance of water was simulated it was quite equal to any mirage I have witnessed on the African deserts.

SYDNEY B. J. SKERTCHLY

Geological Survey, Whittlesea, Cambridge, Aug. 12

Mirages made Easy

THE very interesting account of a mirage in this week's NATURE induces me to send a few observations. The mirage phenomenon is by no means so uncommon in England as many think. Three or four summers ago, on a strip of sand three miles long at Morcambe Bay, I was able to see one almost every hot day, by simply stooping until my eyes were about a yard above the ground. The further part of the sand then appeared as a lake of water, with objects reflected, &c. The nearer edge of this lake receded as the eyes were raised, the whole soon becoming invisible. I saw the same effects last summer off the Holderness Coast, but again only by stooping. At Cambridge I have lately seen a very good lateral mirage, by looking closely

along the surface of a wall fifty yards in length, which had been exposed for some hours to a western sun. Objects near the further end of the wall were distinctly reflected.

CHAS. T. WHITMELL

Trin. Coll. Cambridge, Aug. 4

Science and the Government

THE announcement in your last number of a rumour that the Government is about to withdraw its promise of aid to the Total Eclipse Expedition, seems to bring to a climax the relations of the Government towards science. We can hardly forget that one of the prominent members of the present ministry, and the one considered to have the special control over the spending of the public funds, is member for the University of London. Mr. Lowe was sent to Parliament, irrespective of party considerations, as the representative of a body which thinks it has some claim to a leading place among the scientific institutions of our country. Is it not worth while to consider whether the views of the graduates of London University are represented in the present attitude of the ministry, and whether some representation might not be made to the Government, through the Chancellor of the Exchequer, of the manner in which the present relations between the Government and science are regarded by his constituents?

3, Park Village East, Aug. 20

ALFRED W. BENNETT

AROMATIC GLYCOL*

ALL chemists recollect the profound impression caused by the discovery of glycol in 1855 by M. Wurtz. Up to that time the bodies which were recognised as belonging to the group "alcohol" only included what we now call monatomic alcohols (common alcohol and its analogues), and M. Berthelot hesitated before venturing upon declaring glycerine a triatomic alcohol—an opinion to which Gerhardt never entirely adhered.

M. Wurtz showed that besides ordinary or monatomic alcohols, there are others which, when submitted to certain reagents where ordinary alcohols furnish only one, produce two derivatives. To these substances he gave the name of diatomic alcohols or *glycols*, and recognised that to each monatomic alcohol belonged a corresponding glycol, which only differed from it by the addition of an atom of oxygen. This new view became rapidly extended. It was admitted that belonging to each glycol there was, or might be, a triatomic alcohol or glycerine; that to each glycerine there might be a corresponding tetraatomic alcohol, and so on; these alcohols only differing from one another by the number of atoms of oxygen which they contained, the number being always denoted by the atomicity of the alcohol.

Shortly before M. Wurtz's discovery of glycol, Signor Canizzaro, now Professor at the University of Palermo, but then Professor at Genoa, had announced the discovery of a new alcohol, which he called benzylic alcohol, having to benzoic acid the same relation as vinous alcohol has to acetic acid. He had obtained this substance by the action of potash in alcoholic solution, on the essence of bitter almonds.

Some time later the same chemist discovered that this alcohol might be equally obtained by means of toluol (*toluène*). The method he employed was to subject chlorinated toluol to the action of acetate of potash; and finally to decompose the acetate of benzyl thus obtained by means of potash.

This benzylic alcohol was the starting point of a new series of alcohols known as aromatic monatomic alcohols, and in fact soon afterwards cumylic alcohol was obtained, and Signor Canizzaro himself shortly afterwards published his discovery of tolylic alcohol.

It should, however, be observed that the process by means of which Signor Canizzaro had obtained his benzylic alcohol from toluol, succeeded ill with the homo-

* "Sur un Glycol Aromatique." Par M. Edouard Grimaux.